once been perplexed by finding that the specimens which have been provided for illustration belong to a species differing in some point of structure from the characters which he has given as belonging to the order under consideration. Teachers will also differ as to the relative importance of certain points of structure, and consequently as to the position of some Natural orders. The uncertainty which still hangs over our classification will be illustrated by the following list of points in which the book before us differs from the fourth edition of Professor Babington's "Manual of British Botany," published as recently as 1856: *Droseraceæ* has been moved from Thalamifloræ to Calycifloræ, the genus *Parnassia* being incorporated with *Saxifrageæ; Acerineæ* undergoes the same change of position; *Balsamineæ* and *Oxalideæ* are abrogated as separate orders, their genera being united to *Geraniaceæ; Portulaceæ, Tamariscineæ*, and *Paronychieæ*, on the other hand, are transferred from Calycifloræ to Thalamifloræ, the tribe *Sperguleæ*, however, of the latter order being relegated to *Caryophylleæ; Grossulariaceæ* is no longer found as a separate order, but is united to *Saxifrageæ; Ilicineæ* or *Aquifoliaceæ* changes its quarters from Corollifloræ to Thalamifloræ; *Loranthaceæ* from Monopetalæ to Apetalæ; *Empetraceæ* from Apetalæ to Thalamifloræ; while the apetalous order of *Callitrichaceæ* disappears, its species being found under Calycifloræ united to *Halorageæ;* the hop, on the other hand, is eliminated from *Urticaceæ*, and appears as a separate order, *Cannabineæ;* and Babington's miscellaneous collection of *Amentiferæ* is divided into the four distinct orders, *Salicineæ, Cupuliferæ, Betulaceæ*, and *Myricaceæ; Trilliaceæ, Colchicaceæ*, and *Asparageæ* are combined with *Liliaceæ*. Although these alterations concern chiefly comparatively small and unimportant orders, there is sufficient change to perplex the student, independently of minor re-arrangements of genera, &c. On the whole it will be seen that the tendency is towards the English practice of "lumping," as contrasted with the Continental practice of "splitting," the number of orders of Flowering Plants being reduced from ninety-seven to ninety-two. We are glad, however, to find that this tendency is not carried to the extent we meet with it in Hooker and Bentham's "Genera Plantarum," where *Papaveraceæ* and *Fumariaceæ* are united; we doubt, indeed, whether the interests of students would not have been better served by keeping apart orders with such clear distinctions of outward structure, as far as British species are concerned, as *Saxifrageæ* and *Grossulariaceæ; Geraniaceæ, Oxalideæ*, and *Balsamineæ; Liliaceæ* and *Colchicaceæ*. The re-arrangements of position are no doubt, in nearly all cases, based on correct botanical principles. The difficulty, however, often experienced in drawing up satisfactory diagnoses of the natural orders may be illustrated by comparing those given by such authorities as Hooker and Oliver. In Hooker's "Student's British Flora," for instance, we find the pistil of *Nymphæaceæ* described as "syncarpous;" in Oliver's "Lessons on Elementary Botany," as "apocarpous." Hooker speaks of the stamens of *Oleineæ* as "epipetalous," Oliver as "hypogynous." In the synopsis of natural orders given at p. xiv. of the book under review, mention of the hypogynous stamens in some genera of *Oleineæ* is omitted.

In his analysis of certain difficult and intricate genera, Dr. Hooker has followed the lead of botanists who have made them their special study, as in the case of *Rubus, Rosa*, and *Hieracium*, where Mr. J. G. Baker's descriptions are adopted. This, no doubt, was a wise course in these instances; we regret, however, to find in the whole work so little of the author's own observations; we are sure that in many cases he could have improved greatly on the method of the "London Catalogue," which has been too implicitly followed. This is especially the case with regard to the plants admitted as "colonists" or "denizens." Why should a place be given, for instance, to *Galinsoga parviflora*, found nowhere, we believe, except within a radius of a few miles from Kew Gardens, from which it has escaped? while, on the ballast hills of the north-east coast and some other localities, many plants have apparently become permanently established, of which no mention is made, or their name is merely given in the Appendix. The time of flowering of plants is also one on which little exact observation appears to have been made; one would judge from our hand-books that the only wild flowers to be gathered in December and January are the groundsel and the daisy; while at least a dozen others could be named that are equally, if not more, perennial. We shall look with eagerness for a work embodying a record of recent physiological and morphological observations on British plants, which Dr. Hooker states, in his preface, it was his original intention to have incorporated with the present volume, and which will possess so great a value from his pen.

The specific descriptions in Dr. Hooker's "Student's Flora" are so admirable, terse, and yet sufficient, the arrangement so excellent, and the size so convenient, that it must rapidly become the work in general use, the companion of every botanist during his summer rambles.

ALFRED W. BENNETT

WATER ANALYSIS

Water Analysis: a Practical Treatise on the Examination of Potable Water. By J. Alfred Wanklyn, M.R.C.S., and E. T. Chapman. Second Edition, edited by E. T. Chapman, Member of the Council of the Chemical Society. Pp. 108. (London, 1870.)

IN the preface to this edition we are told that the whole of the last edition has been transferred almost without alteration, but with slightly different arrangement; and that some new matter has been added, consisting of the tetration of waters; a modification of the process for estimating nitrates; a chapter on volatile organic matter; a method of estimating minute traces of lead; and a chapter on the purification of waters. The tetration of waters is the estimation by standard solutions of the amount of acid present in waters contaminated by the refuse of certain factories or in rain water which has fallen near alkali works. We had thought that the word *tetration*, in the preface, was a misprint for *titration* until we found it so spelt in the text; the latter word (or modification), however, appears on p. 38. The modification of the process for the estimation of nitrates consists in treating the water to which caustic soda has been added with a large excess of aluminium scraps, and pouring off the liquid before distillation, instead of permitting all the aluminium to be dissolved, as in Mr. Chapman's original modification of Schulze's process. In the chapter on volatile organic matter, it is pointed out that on distillation of a water with potassic hydrate, there passes over, together with the ammonia, some combined nitrogen, probably in the form of organic bases, and a process is described for its estimation. The method of estimating minute traces of lead by comparison of the coloration produced by sulphuretted hydrogen water in dilute standard solutions of lead with that obtained by the same reagent in the water under examination is not new, for we remember having seen the experiment illustrated in Dr. Hofmann's lectures at least fifteen years ago. The previous removal of oxidising agents by sulphurous acid is, nevertheless, a useful addition. In the chapter on the purification of water, an ingenious experiment is described to show that separation of suspended matter by filtration through sand and similar substances is really due to subsidence within the interstices of the filter.

As in the first edition, we find no mode of estimating sulphides or the gases dissolved; nor does the treatise contain any indication of the success of the application of the ammonia process in the examination of sea water; it may be answered that the title of the book only refers to potable waters, but then we are at a loss to explain the appearance of some experiments on sewage.

It is much to be regretted that the present editor has thought fit to reprint the preface to the first edition, and also the appendix, which consists of nothing but an attack on Frankland and Armstrong's process. These entail on us the necessity of noticing this treatise at much greater length than the positive information contained in it would justify; for if the statements set forth were passed over without remark, it might seem as if the untrustworthiness of the process were acknowledged by all chemists, and doubt thrown on the accuracy and value of the reports issued by the Registrar General. The authors state in the preface to the first edition that it requires great length of time and great skill to execute it. This may be true, but the vast advantages which it possesses over the old processes amply compensate for the additional trouble. The manipulation, though delicate, is not, we are given to understand, beyond the powers of an average first year's student, at least when he is not influenced by a preconceived distrust in the efficiency of the process, or by a desire to do things with as little trouble as possible. It was not quite fair for the authors to state, two months after the publication of the process, that chemists in general agreed with them as to its invalidity. They should have remembered that "chemists in general" are not such rapid workers as Messrs. Wanklyn and Chapman, and would wish for more time to give a definite decision on the capabilities of a process so entirely new. The appendix consists of a note read by the authors before the Chemical Society. They commence by stating that they do not consider the complete conversion of organic nitrogen into ammonia by their method as being essential to its applicability for determining the relative quality of a water, and that they rely simply on the constancy of the ratio between the amount of albumenoid substance in the water and the quantity of ammonia produced. This is a retractation, though not a very straightforward one; for in their first published account of the process it was stated that all the nitrogen was evolved as ammonia. But it would be well to ascertain if this ratio is really constant, for although this may be the case with white of egg, is it not almost too much to assume that the nitrogenous organic matter present in natural water acts in the same manner as albumen, when we are quite ignorant of its proximate constituents? If the quantity of ammonia always bears a certain relation to the organic matter, and if this ratio is known, the determination of the nitrogenous impurity is merely a matter of calculation; but is it not a fact that some bodies, when treated according to the author's process, evolve more ammonia, in proportion to the amount of nitrogen they contain, than do others? The experiments on strychnine, narcotine, and quinine sulphate, published by Frankland and Armstrong (but not commented on by our authors), show this to be the case. But in their published papers, Messrs. Wanklyn and Chapman admit that the quantities of "albumenoid ammonia" from the following compounds are far from uniformly proportional to the amount they contain, thus:—

Urea and Picric acid gave no albumenoid ammonia

Creatine	gave	$\frac{1}{3}$	of its nitrogen as	albumenoid ammonia
Caffeine	,,	$\frac{1}{4}$	,,	,,
Uric Acid	,, about	$\frac{1}{6}$	,,	,,
Albumen	,,	$\frac{2}{3}$	,,	,,
Morphine & 11 other organic substances		$\frac{1}{2}$	,,	,,
Hippuric acid and 7 other organic substances		all	,,	,,

Next follows a list of Frankland and Armstrong's results, in which the differences of the quantities of carbon and nitrogen obtained by experiment and calculations are pointed out. But it should be remembered that in these experiments the substances were weighed instead of being measured in standard solution, and that these solids were first dissolved and the solutions evaporated in order to perform the combustion of the residue. It is afterwards deduced from this same list that the error is inherent in the process, as the results are not better when the amount of organic matter is reduced; but with this reduction the liabilities of error in weighing increase. If this discrepancy were really caused by the imperfection of the process, we should expect to find great variation in the quantities of nitrogen obtained in actual analyses of waters, but in the list of forty nitrogen determinations given in Frankland and Armstrong's paper, the numbers vary from 0·000 to 0·068 per 100,000, or, as our authors would express it, from 0 to 0·68 milligrammes per litre, whereas they accuse the process as being liable to produce an error of no less than 1·29 milligrammes per litre. The following extraordinarily opportune accident has happened during the month of May, and from the results we shall be able to obtain an indication of the concordance of the numbers arrived at by Dr. Frankland's process. In the Registrar General's report it will be seen that the water supplied by the Grand Junction Company was drawn at the cabstand in Woodstock-street, whilst a sample of water was taken at 14, Lancaster Gate, under the impression that it came from the works of the West Middlesex Company. Now, to our certain knowledge, 14, Lancaster Gate is supplied by the Grand Junction Company; thus Dr. Frankland has been analysing two samples of the same water, supposing them to have come from different sources. This mistake could not have been found out till after the report was printed, and the results obtained by the two experiments are as follow:—

	Total solid impurity.	Organic carbon.	Organic nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Previous sewage contamination.	Chlorine.	Total hardness.
Grand Junction, collected at the cab stand in Woodstock Street, May 17.	24·7	·135	·020	·000	·184	·204	1520	1·62	19·36
Water collected at 14, Lancaster Gate, supposed to be from the West Middlesex Company; but actually from the Grand Junction, May 15.	24·6	·129	·023	·000	·188	·211	1560	1·60	19·36

It may be objected that even these numbers do not approximate so closely as those of Messrs. Wanklyn and Chapman, but they represent actual quantities obtained from water, and not a theoretical "albumenoid ammonia," which may not be an indication of the quantity of organic impurities.

Since Frankland and Armstrong's paper was published an immense number of analyses have been made with the process, and in his annual report to the Registrar General, Dr. Frankland states that he has seen no reason to be dissatisfied with the results. Probably no one, unconnected with Dr. Frankland's laboratory, knows better than Mr. Chapman that improvements in the details of the manipulations have been made during the last two years; and it is, therefore, with very questionable taste that he has reprinted the appendix to the first edition of the book without a single word of qualification.

It is much to be deplored that two young chemists, with such undoubted abilities as Messrs. Wanklyn and Chapman possess, should have rendered themselves notorious by attacking older workers in scientific investigation. It is, no doubt, very laudable in a young and ardent investigator, when he points out that high authorities may err and frequently have erred, but the manner in which these gentlemen have carried out their corrections has made their matter more distasteful. It would almost seem as if they found an incentive to work in the hope of being able to overthrow the "huge superstructures" which have been raised by men who have been longer in the field of scientific research.

OUR BOOK-SHELF

A Sketch of a Philosophy. Part III. *The Chemistry of Natural Substances.* Illustrated by two folding plates and 150 figurate diagrams of molecules in the text. By John G. Macvicar, LL.D., D.D. (Williams and Norgate, 1870.)

It is a hard matter to give a just account of this pamphlet. The views propounded by the author are so entirely different from those usually held by chemists, and according to the author's own statement they have been so little studied by others, that it is difficult to know exactly how to treat the subject. We should scarcely be justified in saying that the whole system is mere imagination, though some hold this opinion; but the book, though evidently written with the intensest earnestness, is the work of an enthusiast, which will explain the bitter complaints he makes against modern chemists for not taking more notice of molecular morphology. The author endeavours to explain the formation of all matter by the aggregation of the ethereal element, supposing that all bodies tend to assume a symmetrical, and more or less spherical form. The simplest form of aggregation Dr. Macvicar considers to be the *tetrad*, consisting of four specks of the material element so arranged in space that they form the angles of a tetrahedron, the lines joining them indicating the attracting and repelling forces operating between the units. Two tetrads are also assumed to join base to base producing the *bitetrad*, and from these two forms the tetrad and the bitetrad, all the atoms and molecules of our planet are supposed to be produced. This tetrad by attracting another unit opposite to one of its faces constitutes a group of five units, considered to be the atoms of hydrogen, and with the atomic weight of five. The author proceeds to show the mode of genesis of many other elements and compounds by the juxtaposition of these elemental forms. By calculation he can determine by his system the specific gravities of solids and liquids referred to water as unity, in a manner similar to that by which the densities of gases and vapours may be deduced by the old system. This alone would seem to show that the method deserves more attention from chemists than it has yet received. The non-reception of this molecular morphology may be ascribed to several causes: the diction of the author is peculiar, and he writes in a dogmatic manner, which might be expected in a theological work, but is not usually found in a treatise on natural science; then he pushes his inferences to such an extent (or as some would say, rides his hobby so hard) that his conclusions appear somewhat ludicrous, unsupported as they are by experiment: thus he traces the coincidence between the assumed hexagonal form of the molecule of aqueous vapour and the shape of the minimum snow-flake and ice-flower, and "the inflorescence of plants of the monocotyledonous order, in which an aqueous tissue predominates"; he thinks that one of the forms of aqueous vapour which occupies half the volume of the other, may possibly be converted into the second variety at a high temperature, and thus explain the explosion of steam boilers. Again the dimorphism of water may be the cause of the production of animal heat; for water in the body may be transformed from one of its varieties into the other with evolution of heat; but on escaping from the body as perspiration, the inverse action takes place and cold is produced. But underneath all these extravagances there may be a stratum of truth, and we hope that either the author or some one who understands and accepts his views thoroughly, will so develop them as to ensure their reception by chemists.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Source of Solar Energy

I HAVE not Mr. Proctor's "Other Worlds" by me to refer to, but my impression on reading that book some little time ago, certainly was, that if it did not directly support the *meteoric* theory of solar energy, it at least favoured the idea of innumerable meteors falling into the sun. The principal portion of my letter in last week's NATURE, was not, however, so much addressed against any special views of Mr. Proctor's relative to this *meteoric* theory, as it was against the probability of meteors fall-